AMERICAN JOURNAL of PHYSICS

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Citation: American Journal of Physics **49**, 1120 (1981); doi: 10.1119/1.12559
View online: http://dx.doi.org/10.1119/1.12559
View Table of Contents: http://scitation.aip.org/content/aapt/journal/ajp/49/12?ver=pdfcov
Published by the American Association of Physics Teachers

Vindications of Dirac's electron, 1932–1934

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(Received 18 August 1980; accepted 14 April 1981)

This is the last of a three-part series. The first essay in the series described Dirac's generalizations of quantum mechanics leading to his theory of the electron. The second essay surveyed evaluations of Dirac's theory by other physicists, especially by Bohr who used Dirac's speculations about negative energy electrons as evidence for the failure of quantum mechanics at nuclear dimensions. This essay shows how the material reality of positrons vindicated quantum mechanics and opened new paths for physics.

I. ANOMALOUS GAMMA SCATTERING

The successful use of the Klein-Nishina formula in analyzing the energy loss of gamma radiation was an important test that Dirac's theory passed especially well. However, in the process of testing the Klein–Nishina formula, Meitner and Hupfeld, and others independently, found anomalous scattering as well.[1] Careful experimental studies by Tarrant found results "in excellent agreement with the Klein and Nishina formula" but also found "an additional absorption ... which is presumably nuclear in origin." Chao also found "anomolous scattering" that "originates in the nucleus." L. H. Gray found evidence suggesting that the anomalously scattered gamma radiation was "due to a new process in which incident gammas were absorbed and then not all re-emitted in the forward direction" and "that there is some threshhold for the new process."

Together Gray and Tarrant designed an experiment to detect the gammas scattered backward since these would have the least contribution from normal Compton scattering and thus the greatest relative contribution from the new process.[2] On 14 May 1931 at a Royal Society "Discussion on Ultra-Penetrating Rays," Tarrant reported that he and Gray found that the anomalously backscattered gammas have energies of 1 and 0.5 MeV. They found that independently of the source of radiation and of scattering nuclei, gammas of 0.5 and 1 MeV were backscattered "leading to the view that the radiations are characteristic of some unit of nuclear structure present in all nuclei." Their announcement in 1931 was during a discussion of the origin of cosmic rays that featured Jeans's hypothesis that the source of cosmic rays was "the annihilation of matter." Jeans's argument, which was "being treated with marked respect," explicitly included Dirac's hole interpretation as support. Yet no one recognized Gray and Tarrant's results to be the signature of electron-antielectron annihilation. All the pieces of the puzzle were available in various discussions of the Klein–Nishina formula, the origins of cosmic rays, and nuclear structure. However, since no one suspected that Dirac's antielectron existed, no one even suspected there was a puzzle that the pieces solved.

All would have come clear quickly by looking at anamolous gamma scattering "using a cloud chamber operated in a magnetic field to study the secondary electrons produced in a thin lead plate inserted in the cloud chamber" as proposed by a graduate student working in the room next to Chao's at Caltech in 1929; however, Millikan had other ideas and would not allow Anderson to continue his experiment.[3]

II. ANDERSON'S DISCOVERY

In May of 1931 Jeans lectured on "The Annihilation of Matter" at Princeton, Yale, and Harvard. He argued "that stellar radiation is produced by spontaneous annihilation of matter ... in which proton and electron both disappeared in radiation." Millikan meanwhile promoted another theory. In his presidential address to the BAAS in December of 1930 he conceded that electrons and protons do "arrange a suicide pact ..., jump into each other's arms ..., and ... are snuffed out at once ... letting loose a terrific death yell." Since cosmic ray measurements showed no evidence of the terrific death yell let loose in this annihilation, Millikan argued the process must be confined to the interior of stars. Cosmic rays, he claimed, are the birth cries of "the building up in the depths of space of the commoner heavy elements out of hydrogen." The annihilation and creation hypotheses had long histories and nurtured several major controversies. In order to test his hypothesis more carefully, Millikan assigned Anderson the task of investigating the energies of incident gamma radiation by measuring the energy of Compton-scattered electrons from the curvature of their tracks in a cloud chamber in a magnetic field, a method developed by Skobelzyn.[4]

Beginning in 1930 Millikan and Anderson designed a cloud chamber to operate between the pole pieces of a strong (about 20 kG) electromagnet. The curvature of electron tracks was measured from photographs taken through a hole through a pole piece. In 1932 Millikan and Anderson reported that "particles of positive charge appear as well as electrons." Millikan was convinced, and Anderson dutifully reported that "the positives are protons." Anderson, however, had some doubts: Because of the experimental conditions good information about the mass of the unexpected positives using specific ionization along a track could be obtained only when the particles were slow, and most of the low-velocity tracks were too short to have been made by protons. Anderson thought the downward-moving "positives" might be upward-moving electrons. To resolve the "heated" difference between Anderson and Millikan a lead plate was inserted into the cloud chamber that would show the direction of movement by loss of energy of the particles traversing the plate. Shortly Anderson found a low-energy, low-mass, positively charged particle moving upward in the chamber.[5]

Anderson's letter published in *Science* on 9 September 1932 announced his observation of "a positively charged particle having a mass comparable with that of an electron." *Science News Letter* (24 September 1932) announced

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"the probable existence of a new particle of matter." Anderson published a less reserved fuller report of "The Positive Electron" in March of 1933, announced observations of "free positive electrons resulting from the impact upon atomic nuclei of the protons from ThC" in May, and christened his discovery "positron" in a report of pair production in June.[6]

Anderson knew of the more advanced work in quantum mechanics—he had remained in Oppenheimer's course while the others had dropped out—and Oppenheimer recalled discussing Dirac's hole interpretation with Anderson. However, Oppenheimer himself believed that *all* Dirac's energy states were completely filled and that holes could not exist or be observed. Anderson's apparatus was not designed to test Dirac's hole interpretation and only accidentally provided barely adequate conditions for observing positrons. Indeed, Anderson did not connect his discovery with Dirac's theory until September of 1933, after many others had, and then only to call attention to a new absorbtion process (pair production) to be used in investigating the energies of incident gamma rays as required by Millikan's program. Anderson maintained this attitude even in his triumphant address to the American Physical Society at the end of 1933 and in his Nobel Lecture in 1936. It was left to R. M. Langer at Millikan's Norman Bridge Laboratory to speculate on the relationships between the experiments and Dirac's theory.[7]

III. BLACKETT AND OCCHIALINI'S CONFIRMATION

P. M. S. Blackett was assigned to be Rutherford's research student in 1921 and given the task of making a cloud chamber photograph of the artificial disintegration. $N + \alpha \rightarrow O + p$, discovered by Rutherford two years earlier. "After two or three years of carpentry, machine work, electric wiring" Blackett obtained his famous photograph. There were, however, serious differences between Rutherford, who wanted quick results pertaining to the nucleus without developing technique beyond the needs of the task, and Blackett, who sought to master all parts of cloud chamber technique and use this in various parts of physics, such as testing Mott scattering. In 1932 G. P. S. Occhialini joined Blackett bringing the technique of coincidence counting developed by B. Rossi and his group in Italy: "The marrying of the counter technique with the cloud chamber was an obvious step." Before long Blackett and Occhialini reported successful operation of a coincidence triggered cloud chamber for study of cosmic rays. Difference between their program and Anderson's, and a sense of competition, were noted in *Nature*. Blackett and Occhailini's chamber was triggered by coincidence of radiation passing through Geiger counters above and below the chamber that increased greatly the number of photographs with measurable events over that produced by Anderson's random operation. Anderson had to photograph through a pole piece of a strong magnet while Blackett and Occhialini used water-cooled coils arranged to provide a more uniform 3 kG over a larger chamber and photographed with two cameras, one 20° off axis, allowing three-dimensional reconstruction of events.[8]

Anderson discovered the positron as a result of puzzling over unusual events recorded in an experiment designed to test Millikan's atom-building speculation. Blackett and Occhialini immediately confirmed the existence of Dirac's positive electron with more and higher-quality evidence produced by an apparatus designed as a general tool. They could measure curvature, range, and ionization more accurately and were able to determine more definitely that the positive particles were not protons but had the same mass as the electron. Because they could reconstruct their tracks more completely they could argue more convincingly that positive and negative electrons were created at the origin of the diverging tracks. They clearly and fully identified the positive electron with Dirac's antielectron, worked closely with Dirac, and give his calculation of the "simultaneous annihilation of a positive and negative electron ... which predicts a time of life for the positive electron that is long enough for it to be observed in the cloud chamber but short enough to explain why it had not been observed by other methods." Furthermore, they recognized immediately that the anamolous absorbtion of gamma radiation studied by Gray and Tarrant, Meitner and Hupfeld, and Chao "may be connected with the formation of positive electrons and the re-emitted radiation with their disappearance."[9]

Rutherford's right-hand man Chadwick now joined Blackett and Occhialini and they began to study the production of positive electrons from nuclei. Duplicating the conditions in which Curie and Joliot had observed "retrograde electron tracks in an expansion chamber" seeming to enter a beryllium target while it was exposed to alpha radiation from a polonium source, and using Blackett's stereoscopic and statistical techniques they reported "statistical examination of the results supports the view that the tracks began in the target and therefore carried a positive charge." By early 1934, when they gave a full report of their various experiments indicating that electron–positron pairs are created by gamma radiation in the Coulomb field of nuclei, there was considerable evidence from Anderson in the U.S., Curie and Joliot in France, Meitner, Phillip, and Kunze in Germany, and themselves in England of the production of positrons.[10]

No one at the Cavendish had expected that Dirac's antielectrons would be observed, not even Dirac. Blackett took the lead in relating the unexpected discovery to Dirac's theory. In September of 1933 he reviewed the accumulating evidence for the positron at the BAAS meeting. Again at the Solvay conference in October he reviewed the evidence and argued forcefully that it confirms Dirac's theory of the electron. In the extended discussion following Blackett's remarks Bohr seconded Blackett's argument that Dirac's theory had been vindicated by the new properties of matter disclosed by experiment. Rutherford, however, belittled the connection between experiment and theory made by Blackett. In March Rutherford thought that the positively charged particles only "may prove to be the positive electron" but by November he was sure of electron–positron creation. By then Blackett had left the Cavendish for Birkbeck College, London, partly because of differences with Rutherford, partly to reduce his teaching duties, partly to have his own show to run, and, no doubt, for other reasons as well. Although Blackett most completely connected the discovery of the positron with Dirac's theory and argued most insistantly that this was a most fundamentally important result in his inaugural address at Birkbeck, as at the Solvay Conference, he also announced his own intention to concentrate on investigations of cosmic rays especially of the phenomena of showers "a most striking result" of the original work with Occhailini.[11]

IV. CURIE AND JOLIOT'S NEW EVIDENCE

Results of Irene Curie and Fredrick Joliot's studies of nuclear processes induced in beryllium by alpha particles from a polonium source had been interpreted and developed by Chadwick as evidence for the existence of the neutron. To study the energetics of the induced radiations of beryllium, Curie and Joliot studied the energy spectrum of Compton-scattered electrons and saw tracks that were interpreted by Chadwick, Blackett, and Occhialini as new evidence for the positron.

The French team sought to resolve the question of the origin of the positrons[13] and in May argued that their experiments indicated that positron–electron pairs were created by gamma radiation as a direct materialization of energy, though they did suggest another interpretation in which a "dislocation" of the "neutrino de Pauli–Fermi" might also produce a positron–electron pair. In July they reported experiments indicating another process and argued that the transformations

$$B^{10} + \alpha \rightarrow C^{13} + e^+,$$
$$Al^{27} + \alpha \rightarrow Si^{30} + e^+$$

involved the production of positrons by protons,

$$p \rightarrow n + e^+,$$

indicating "la complexite du proton."

Curie and Joliot summarized their work on the production of positrons by energy materialization and by nuclear trasmutations in August and again in October at the Solvay Conference. Other reviews were published by Bothe and Kunze in November and December. By the end of 1933, it was clear that positrons were not at all rare: their properties and their role in nuclear processes were hot topics of study. There were, as well, abundant speculations about the higher purposes of the positron including suspicions that they represented the long sought materialization of the ether.[14]

In January of 1934, Curie and Joliot reported their own great discovery: "Our latest experiments show a very striking fact: when an aluminium foil is irradiated on a polonium preparation, the emission of positrons does not cease immediately when the active preparation is removed." Within a year more than fifty new radioactive isotopes had been created using alphas, deuterons, protons, and neutrons. Artificial radioactivity was a rich field for those with accelerators. M. Stanley Livingston remembers that artificial radioactivity was created with Lawrence's cyclotron within one-half hour of learning of Curie and Joliot's discovery merely by rewiring a switch so that counters remained on after the accelerator was turned off. But accelerators were not necessary: Shortly after Curie and Joliot's discovery, Fermi announced very impressive preliminary results of using neutrons to create radioisotopes systematically up the periodic table by his group in Rome and by fall had raised the problem of transuranic elements that led to the discovery of fission.[15]

V. FERMI'S BETA DECAY

The easy, abundant, and diverse appearances of the positron exacerbated the problem of nuclear electrons and Bohr reiterated his fears about the limits of quantum mechanics at the 1933 Solvay Conference. On the other hand, the positron offered rich new possibilities for speculation. In the summer of 1933, Guido Beck used pair production to resolve the Klein paradox for nuclear electrons and the continuous beta-decay spectrum.[16] Following suggestions by Bohr, Beck used "a more general application of the conservation laws" to loose energy and momentum in the nucleus and thus considered the "unknown particle which it is proposed to call a 'neutrino' ... an unnecessary complication."

At the 1933 Solvay Conference, Pauli responded to Bohr's renewed interpretations of the continuous beta-decay spectrum that "admits that the laws of conservation and momentum do not hold when one deals with a nuclear process where light particles play an essential part" remarking that "this hypothesis does not seem to me either satisfactory or even plausible." Pauli believed there were good reasons to use conservation laws and wondered why Bohr considered conservation of charge valid but not conservation of energy and momentum. Now Pauli more boldly presented his neutrino hypothesis. His remarks were followed by reports by Chadwick and Meitner of independent experiments that failed to detect the existence of the neutrino and by the claim of F. Perrin that the shape of the beta-decay spectrum implied the neutrino's "*zero intrinsic mass* like the photon." Also at the conference Joliot had reiterated the earlier suggestion of Curie and Joliot that the Pauli–Fermi neutrino might be associated with creation of electrons and positrons.[17]

Emilio Segre remembered:

> "After the Solvay conference, Fermi returned to Rome, ruminated on the theory of beta-decay, and decided that he had to learn second quantization. He had bypassed creation and annihilation operators in his famous electrodynamics article, (Fermi, 1932, 67) because he could not make them our very well. Now, in 1933, he decided he had to understand them. So he sat down and studied them. Then he said: "I think I have understood them. Now I am going to make an exercise to check whether I really understand them, whether I can do something with them.' An he went on to set forth his theory of beta-decay, which in his own estimation was probably the most important work he did in theory. He told me that he thought that this would be the discovery for which he would be remembered."

The basic assumptions of Fermi's theory were that the nucleus consists only of heavy particles existing in neutron or proton states and that with each transition from a neutron to a proton state, two light particles—an electron and a neutrino—are created *de novo*. He thus conserved energy, momentum, and charge, but did not conserve identity and number of particles. His model for the creation, or annihilation, of an electron and neutrino was not the creation or annihilation of an electron–positron pair in Dirac's theory of the electron, but the creation of photons from the vacuum in Dirac's quantum theory of radiation. To represent his beta-decay process, Fermi used the wave functions of Dirac's theory of the electron and the creation and annihilation operators of Dirac's theory of radiation.[18]

In the spring of 1934, Hans Bethe and Ruldolf Peierls wrote to *Nature* that the beta-decay electron and Pauli's neutrino "could be described either (a) as having existed before in the nucleus or (b) as being created at the time of emission." Fermi's theory, they noted, used (b) and "seems to be confirmed by experiment." According to (b), they continued, "the role of neutron and proton would be symmetrical" as suggested by Curie and Joliot at the Solvay Conference. Their subsequent discovery of artificially radioactive positron emitters gives "strong support to method (b), as one can scarely assume the existence of positive electrons in the nucleus." Fermi's student Gian Carlo Wick had already published his extension of Fermi's theory of beta decay to cover the positron case. Fermi's creation theory of beta decay got electrons, negative and positive, out of the nucleus and was, as Weisskopf put it, "the first example of modern field theory."[19]

VI. SECOND THOUGHTS

Anderson's discovery, Blackett and Occhialini's confirmation, Curie and Joliot's new evidence, and Fermi's use vindicated Dirac's relativistic quantum dynamics. However, the material reality of the holes in negative energy states aggravated the old problem of the relation between matter and radiation. At the 1933 Solvay Conference, Dirac discussed the problem of the infinite field due to the negative energy states and the polarization of the vacuum by creation and annihilation of electron–positron pairs. Peierls, then working with Dirac, studied this by a different method and also noted the serious problem. The central problem for theoretical physics was to make a field theory of electromagnetic phenomena out of photons and electrons, and the positron complicated an already difficult task. Understandably, Dirac did not call attention to these problems in his Nobel Lecture in December of 1933. Other than the second edition of his *Principles*, Dirac's only published paper in 1934 was a "Discussion of the Infinite Distribution of Electrons in the Theory of the Positron," sometimes called the beginning of "subtraction physics." Dirac lectured on quantum electrodynamics at the Institute for Advanced Study in 1934–1935. These lectures show his growing disaffection for a theory that "has never as yet given any result not previously obtained otherwise" and that is troubled with fundamental divergences: "Just as the self-energy of the electron can be regarded as due to many nascent light quanta surrounding it, so the theory gives around each photon many nascent electrons and positrons which give it a self-energy ... and this turns out to be infinite."[20]

Problems of quantum electrodynamics were the central issue at an international congress on theoretical physics at Cracow in the summer of 1934. In the Fall of 1935 and Winter of 1936 Pauli led a seminar at the Institute for Advanced Study on "The Theory of the Positron and Related Topics," which reviewed the problems aggravated by Dirac's antielectron. The seminar began with Pauli's report on the Pauli–Weisskopf "*Anti-Dirac Theory*!," which gave a physical interpretation to the Klein–Gordon equation. In a guest lecture at another Institute seminar, Pauli put the situation pungently:

> "The quantum theory is very satisfactory as it stands in two fields: (1) in the non-relativistic theory of matter, where it gives excellent results in field of spectra, atomic collisions, etc.; and (2) in the theory of the electro-magnetic field so long as one does not try to treat the sources of the field as well, where it gives a correct theory of the transmission, interference, and particle-like nature of light. In other words, it works very well as long as we can treat one or other of the field or matter as a given "external" influence, but when we try to unite the two to give a theory describing the interaction, we find that it is not at all so satisfactory.
>
> This is closely connected with Dirac's attempt to give a real relativistic theory of the electron. In this attempt, the success seems to have been on the side of Dirac rather than of logic. His theory consisted in a number of logical jumps. First, he made a theory for *one* particle; this is allowed so long as the number of particles in a system is conserved. From certain postulates he derived an equation which describes the relativistic motion of an electron, and in a natural manner, the spin of the electron, and its magnetic moment. But the equation allowed the electron to be in states of negative energy, and if one allowed it to interact with the electromagnetic field, there was nothing to stop it from making transitions to these states. To avoid this conflict with observation, Dirac made a logical jump; he assumed that all the negative states were filled up with electrons, and then transitions to them were forbidden by the exclusion principle. This leads to the theory of holes for the positron, which I do not like at all. There is no longer a conservation of the total number of particles, when one considers positrons, and so Dirac's argument for the form of the wave equation is no longer cogent because then there no longer exists any *a priori* reason that the wave equation shall be of the first order and the charge density shall be a sum of squares ....
>
> It seems that quantum theory is always successful when describing systems with a finite number of degrees of freedom, but that when dealing with systems possessing infinitely many degrees of freedom, it causes divergent results to appear. The field in a vacuum possesses an infinite number of degrees of freedom, since it is equivalent to an infinity of oscillators, but in this case, the infinities take on a fairly harmless form. It turns out that the energy of the field is infinite, but one may subtract this infinity in a well-defined way, and get finite results for the *difference* of the energies of two different states of the system. One already sees here the beginning of the 'subtraction physics' which is such an unsatisfactory feature of the present theory....
>
> The theory of holes postulates an infinite number of electrons, and therefore, comes into the same category. Here the infinities are far worse. Not only is the energy infinite, but also the polarizability of the vacuum. An external electron field will create pairs of electrons and positrons, which polarize the vacuum, and a charged particle will surround itself with the particles of opposite charge created by its field; the theory leads to infinite results for those phenomena.
>
> At the present moment, J. von Neumann is trying to develop a theory which will avoid these difficulties for systems with infinitely many degrees of freedom, with a different concept of state for such systems."

Pauli's seminar concluded with von Neumann's paper on the quantum mechanics of infinite systems in which he showed how indeed divergences must arise in any theory of systems with infinitely many degrees of freedom.[21]

In January of 1936, Robert S. Shankland reported his repetition of the Bothe–Geiger–Compton–Simon experiment, which in 1924 had established the reality of photons and quashed the Bohr–Krammers–Slater suggestion that energy is not conserved in atomic phenomena. Shankland used high-energy gamma radiation and electronic coincidence counters but, unlike his predecessors, did not find coincidence between the Compton-scattered photon and electron. Dirac took this occasion to ask "Does Conservation of Energy Hold in Atomic Processes?" in *Nature*. He argued that Shankland's result, along with nuclear processes such as beta decay, "suggest that we take as the starting-point in our reformulation of atomic theory, the assumption that energy and momentum ... are not in general conserved in processes involving large velocities, including radiative processes." His real problem was that quantum mechanics "loses most of its generality and beauty when one attempts to make it relativistic." He was using Shankland's result to justify his conclusion that because of the ugliness and limitations of subtraction physics, "the so-called quantum electrodynamics must be given up .... We may give it up without regrets—in fact, on account of its extreme complexity, most physicists will be very glad to see the end of it." Bohr's colleague, J. C. Jacobsen repeated Shankland's experiment with results that "seem to confirm the usual theory of the Compton effect in every detail." Jacobsen's report in *Nature* was followed immediately by the letter from Bohr, "Conservation Laws in Quantum Theory." In response to Dirac's question about conservation, Bohr argued that the remaining limitation on quantum mechanics was the atomicity of charge and that resolution of this would require no "real departure from the conservation laws of energy and momentum." Bohr no longer had any worries about nuclear physics because "the grounds for serious doubts as regards the strict validity of the conservation laws in the problems of the emission of β rays from atomic nuclei are now largely removed by suggestive agreement between the rapidly increasing experimental evidence regarding β ray phenomena and the consequences of the neutrino hypothesis of Pauli so remarkably developed in Fermi's theory."[22]

What had appeared to be a problem with dynamics had been resolved by the existence of a new form of material reality. From 1932 to 1937, four new elementary particles materialized—neutron, positron, neutrino, and meson—to ratify the generality and usefulness of quantum dynamics. Dirac's worries were not at all typical, for confidence about the progress of physics, especially experimental physics, led more and more by Fermi, was growing.

Dirac's electron opened important new paths for physics that we are continuing to explore. Dirac has continued his independent search for more beautiful generalizations in reconsiderations of classical theory, in examinations of the use of new mathematical generalization to make relativistic wave equations, and in explorations of connections between atomic theory and cosmology. In February of 1939, he presented his most Olympian view of natural philosophy discussing the "Mathematical Quality in Nature" by which we can know nature, the "Principle of Mathematical Beauty," which determines the form of nature, and the "Unity of Nature" by which material reality follows mathematical form.[23]

Meanwhile, across the Atlantic, more practical matters dominated as Fermi was constructing experiments on self-sustaining nuclear fission reactors and Bohr was calculating fission probabilities.

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Aerodynamic effects on discus flight

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(Received 30 October 1980; accepted 22 January 1981)

Skilled discus throwers claim that a properly thrown discus will travel several meters farther if it is thrown against the wind, than if it is thrown along the direction of the wind. Numerical calculations confirm these claims for winds of up to about 20 m/sec and show that the extra distance is caused by the higher lift and drag forces acting on a discus that is thrown against the wind. Aerodynamic considerations influence numerous aspects of discus throwing, but these have not been dicussed in the scientific literature. In addition to reviewing the available literature, the present article calculates the effect on distance thrown caused by changes in wind velocity, altitude, air temperature, gravity, and release velocity. Some sample results are that a discus can travel: (i) 8.2 m farther against a 10-m/sec wind than with such a wind; (ii) 0.13 m farther at 0 °C than at +40 °C; (iii) 0.19 m farther with no wind at the elevation of Rome, Italy than at the elevation of Mexico City, Mexico; and (v) 0.34 m farther at the equator than at the poles.

INTRODUCTION

Wind drag is an important factor affecting performance in a number of individual sports, including bicycle racing, track running (sprinting), and long jumping. Generally, for best performance it is advantageous to be moving in the same direction as the wind. In an attempt to nullify this advantage, records are disallowed in certain track and field events if there is too large a component of wind velocity along the direction of the run or jump.

Discus flight is also influenced by wind, but unlike most other track and field events, *discus throwers can throw significantly farther if the wind blows against the direction of the throw than if there is no wind or if the wind blows in the same direction as the throw*. When thrown properly, a discus is an airfoil, and the aerodynamic lift more than compensates for the loss of performance due to drag. Discus enthusiasts have been aware of this paradoxical result for many years. Almost 50 years ago Taylor[1] measured drag and lift coefficients for a discus, calculated a few trajectories, and recommended that record performances be "adjusted" for the effect of the winds. His recommendation was not instituted, and to this day discus records are allowed under any wind conditions.

Suprisingly enough, there are apparently no physics textbooks or articles in scientific journals that discuss the aerodynamics of discus flight. In fact, most of the investigations of the aerodynamics of discus flight have been reported in exceedingly obscure places. For example, the most widely quoted numerical calculation of the effect of air on discus flight is the unpublished work of Cooper *et al.*,[2] who performed their analysis as a class project for an engineering course at Purdue. Several important studies, including the best discussion on the effects of discus rotation on discus flight[3] appeared in *Discobulus*, which was a mimeographed newsletter in the 1950s for a club of British discus enthusiasts. The most comprehensive work on discus aerodynamics is available only in Russian.[4] Measurements of the drag and lift coefficients for a discus at various angles of attack have been published in a physical education journal by Ganslen.[5] Several other authors have presented summaries of portions of the above work, including Lockwood,[6] Dyson,[7] and Hay.[8]

Many basic questions about the effect of physical variables on discus flight are not addressed at all by any of the previous work. As an airfoil, how will the discus be affected by changes in air density, Earth gravity, and its own mass and shape? Will a discus perform better when thrown at high altitudes and high air temperatures or at low altitudes and colder air temperatures? How much further will a discus travel if thrown at the Earth's poles than at its equator? If they are released at the same velocity, which will travel further, a men's discus or the smaller and lighter women's discus (see Table I).

FACTORS INFLUENCING DISCUS FLIGHT

Definitions of basic variables

When in flight, a discus is affected only by the forces of gravity, aerodynamic drag, and aerodynamic lift (see Fig. 1). If there is wind with nonzero velocity $\mathbf{v}_w$ then the aerodynamic drag will not act along a direction opposing the velocity $\mathbf{v}_d$ of the discus, but rather it will act along the direction of the relative velocity $\mathbf{v}_{\rm rel}$ (see Fig. 1) where

$$\mathbf{v}_{\rm rel} = \mathbf{v}_d - \mathbf{v}_w. \qquad (1)$$

The magnitude of the drag and lift forces are usually represented in terms of the dimensionless drag and lift coefficients c_d and c_L:

$$F_{\rm drag} = \tfrac{1}{2} c_d \rho A v_{\rm rel}^2; \quad F_{\rm lift} = \tfrac{1}{2} c_L \rho A v_{\rm rel}^2, \qquad (2)$$

where ρ is the density of the air and A is the maximum

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